

# **EXHIBIT 10**

# Why Birthright Citizenship Matters for Immigrant Children: Short- and Long-Run Impacts on Educational Integration

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This paper examines whether the introduction of birthright citizenship in Germany affected immigrant children's educational outcomes at the first three stages of the education system: preschool, primary school, and secondary school. Using a birth date cutoff as a source of exogenous variation, we find that the policy (i) increased immigrant children's participation in noncompulsory preschool education, (ii) had positive effects on key developmental outcomes measured at the end of the preschool period, (iii) caused immigrant children to progress faster through primary school, and (iv) increased the likelihood of them attending the academic track of secondary school.

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## I. Introduction

Children with migration backgrounds constitute the fastest-growing segment of the population in many countries across the developed world, shaping these societies for the future (Tienda and Haskins 2011; Dustmann, Frattini, and Lanzara 2012). Immigrant families bring many strengths to their host countries, but they also present serious policy challenges. A core concern, hotly debated by scholars and policy makers alike, is the educational performance of immigrant children. Although there are some countries where certain groups of immigrant children perform at least as well educationally as children of the native born, there are many others where they are, on average, outperformed by their native peers in school (Algan et al. 2010; Dustmann, Frattini, and Lanzara 2012; Diehl, Hunkler, and Kristen 2016). In Europe, for example, considerable evidence suggests a substantial immigrant disadvantage in multiple indicators of academic achievement. Unsurprisingly, tackling this disadvantage has become a key priority for many governments. However, relatively little is known about which interventions could be effective in fostering immigrant children's educational integration.

In this paper, we examine an important instrument of integration policy: immigrant children's access to host-country nationality. Specifically, we exploit a natural experiment in Germany that saw the introduction of conditional birthright citizenship. Through the reform, a significant portion of immigrant children automatically acquired German nationality and thus the same legal rights—and as such political and professional opportunities—as their native counterparts. From this, as we will argue in greater detail below, it can be hypothesized that the reform increased the returns to education for immigrants, created incentives for human capital investments, and thus fostered immigrant children's educational integration.

To assess the empirical relevance of this hypothesis, we adopt a dynamic view of the education process and analyze the impact of birthright citizenship

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on a series of educational indicators measured at the first three stages of the German education system: preschool, primary school, and secondary school. Some specific questions we address include the following: Did the introduction of birthright citizenship affect immigrant children's participation in noncompulsory preschool education and their developmental outcomes at this educational stage? Did it accelerate their progress through primary school? Was there an effect on immigrant children's selection into different secondary school tracks? Taken as a whole, the answers to these questions allow us to gauge the extent to which granting citizenship to immigrant children at birth affects their educational outcomes in both the short and the long run.

The citizenship reform we examine was introduced in Germany at the turn of the millennium; it essentially constituted a change from *ius sanguinis* (right of blood) to *ius soli* (right of soil). Under *ius sanguinis*, only descendants of home-country nationals receive citizenship. By contrast, under *ius soli*, every individual born on the national territory is eligible for citizenship.<sup>1</sup> As a result of the German reform, all children born to foreign parents from January 1, 2000, onward were automatically granted citizenship if at least one parent had been a legal resident in Germany for eight or more years at the time of birth. This specific feature provides us with a birth date cutoff regarding the entitlement to birthright citizenship.<sup>2</sup> We rely on this exogenous source of identification and employ a difference-in-differences design that compares immigrant children born shortly before and shortly after the cutoff date and draws on immigrant children from the preceding birth cohort as a control group—an approach that allows us to net out possible age and seasonal differences between children born in different semesters.

Two unique administrative data sets covering the student population of one federal state of Germany, Schleswig-Holstein (a state with 2.9 million inhabitants), allow us to implement our empirical approach and to provide answers to the questions raised at the outset. First, we use administrative records from school entry examinations (SEEs). These records contain physicians' assessments of children's language proficiency, socioemotional maturity, and overall school readiness at age 6. An accompanying questionnaire, completed by parents, provides (retrospective) information on children's preschool enrollment and family background. Our baseline analysis relies on the

<sup>1</sup> Birthright citizenship has historically been in place in the United States, Canada, and the United Kingdom but has only recently been introduced in some European countries (e.g., Belgium, Germany, Greece, and Portugal), while in others, its introduction has been discussed (e.g., Austria and Italy).

<sup>2</sup> The reform also changed other features of Germany's nationality law, which we discuss in sec. II.A.

records of immigrant children born between July 1998 and June 2000 and examined for school entry in 2005 and 2006. Second, we rely on administrative school registers (SRs). These registers contain information on children's progress through primary school, primary teachers' track recommendations, and children's selection into secondary school track. We use SRs from 2009 and 2010, the time when the cohorts under study were scheduled to start secondary school.

In addition to being one of the main immigrant destination countries, studying the case of Germany has at least one further advantage. In the German education system, there are essentially three parental decisions to be made: (i) whether to enroll a child in preschool, (ii) when to enroll a child in compulsory primary school, and (iii) which educational track a child will follow in secondary school. In making the latter two decisions, parents can draw on official recommendations. In particular, prior to primary school enrollment, pediatricians assess a child's school readiness and may recommend deferred school entry. In a similar vein, teachers make a recommendation for a child's secondary school track in the final year of primary school. In several federal states, including Schleswig-Holstein, official recommendations are meant to guide parental decision-making but are nonbinding—that is, the educational decisions are at the parents' discretion. As such, we are able to ask whether (possible) effects of the reform are driven by changes in official recommendations, parental educational choices, or a combination thereof.

We obtain three sets of results related to the different educational phases. The key result for the preschool period is that the introduction of birthright citizenship led to a 3.3% increase in immigrant children's enrollment in noncompulsory preschool and, thus, to basically full preschool attendance among immigrant children enjoying birthright citizenship. Turning to developmental outcomes measured at the end of the preschool period, we find a significant increase in immigrant children's German language proficiency and their socioemotional maturity (of 6.0% and 2.1%, respectively). During the primary school phase, our results suggest the following impacts of birthright citizenship: the average school starting age of immigrant children decreased by 1.6 months, or, in other words, the incidence of early school entry more than doubled. Regarding educational progress, we find a 24.5% reduction in the probability of grade retention among immigrant children—an effect that closes the immigrant-native gap by more than one-half. Finally and most importantly for immigrants' longer-term outcomes, we detect a significant increase in immigrant children's probability of attending the academic track of secondary school of 38.9%—an effect that closes the academic track enrollment gap between immigrant children and their native peers by almost one-half. Importantly, this effect is largely driven by an adjustment in immigrant parents' secondary school track choices and not by improved teacher recommendations.

We view our study as making three contributions to the literature. First, we contribute to an emerging literature identifying the causal determinants of immigrant children's educational integration. A few recent studies address the integrative impact of educational policies, such as granting immigrant children access to preschool (Currie and Thomas 1999; Drange and Telle 2017; Felfe and Huber 2017; Cornelissen et al. 2018; Felfe and Lalive 2018) or offering immigrant students a tutoring and career counseling program (Carlana, Ferrara, and Pinotti 2018).<sup>3</sup> We instead focus on the educational effects of birthright citizenship, a low-cost but contentious integration policy. Second, we recognize that education is a dynamic process that proceeds in stages and provide a nuanced picture of the effects of birthright citizenship as immigrant children progress through the first three critical stages of this process. In contrast, previous work has typically focused on one stage of this process in isolation. In fact, much of the literature examining ethnic educational gaps relies on data from PISA and thus focuses on the secondary education stage only (e.g., Dustmann, Frattini, and Lanzara 2012).<sup>4</sup> Finally, our setting allows us to study immigrant parents' choices regarding their children's education. As such, we contribute to the current political and scientific discussion on how to overcome one of the main obstacles to immigrant children's educational integration: their parents' behavior, which is frequently characterized by a lack of effective interactions with schools and other institutions that promote the development of their children (Diehl, Hunkler, and Kristen 2016).

The remainder of the paper is organized as follows. In the next section, we describe the institutional background and discuss potential mechanisms linking birthright citizenship to immigrant children's educational integration.

<sup>3</sup> We discuss these studies in greater detail in sec. V.E. Some related studies conduct cross-country comparisons of test scores resulting from the Programme for International Student Assessment (PISA) and relate better test performance to school systems with reduced ability tracking and a low share of private schools (Schneeweis 2011; Cobb-Clark, Sinning, and Stillman 2012; Ruhose and Schwerdt 2016).

<sup>4</sup> The same is true for a contemporaneously written technical report by Clots and Sajons (2014). Although the issues we are interested in are similar to those explored in Clots and Sajons (2014), our approach differs markedly from theirs. Most important, the administrative data we have at hand allow us to study the reform's effect from an early life-cycle perspective, i.e., across the first three critical stages of the German education system. Clots and Sajons (2014) draw on survey data from the National Education Panel Study (NEPS) and focus on the secondary school stage in isolation. Second, we analyze not only children's educational outcomes but also official recommendations regarding children's educational career. Finally, our identification strategy compares immigrant children born shortly before and after the reform's cut-off date and draws on immigrant children from adjacent birth cohorts as a control group. In contrast, the identification strategy chosen by Clots and Sajons (2014) relies on a comparison between immigrant and native children.

Section III introduces the empirical strategy, and section IV describes our data sources. Section V presents the main results, provides sensitivity checks, and places our findings in perspective by comparing the costs and benefits of birthright citizenship to those of alternative policies. Section VI concludes the paper.

## II. Institutional Background and Potential Mechanisms

### A. Reform of the Nationality Law in 1999

In May 1999, the German parliament undertook a major revision of the German Citizenship and Nationality Law that had dated back to 1913. On July 15, 1999, the German parliament ratified the new version of the law, which included two major changes: (i) it adjusted the eligibility criteria for naturalization and (ii) it introduced birthright citizenship. This paper focuses on the second change, summarized in table 1.

Until 1999, citizenship was granted according to *ius sanguinis*—that is, children became German citizens only in cases in which at least one parent held German citizenship. The legal status of immigrant children born to non-German citizens was either that of a temporary or that of a permanent resident.<sup>5</sup> As of January 1, 2000, the prevailing regime changed to *ius soli*, granting each child born in Germany a conditional right to German citizenship at birth. The conditionality attached to birthright citizenship was that at least one parent had been a legal resident in Germany for at least 8 years at the time of childbirth. On satisfying this condition, German citizenship was automatically registered in the child's birth record with no need for the parents to apply for it but also with no right to disclaim it.

Parents of children born between 1991 and 1999 could make use of a transition rule: from January 1 to December 31, 2000, they could apply for their children's citizenship conditional on having legally resided in Germany for at least 8 years. However, only a small fraction of eligible families (approximately one-sixth) made use of the transition rule.<sup>6</sup>

<sup>5</sup> According to the NEPS, among immigrant children born to non-German citizens in 1999, the shares of permanent and temporary residents amounted to 80% and 20%, respectively. Although citizenship and permanent residency allow individuals to live in Germany indefinitely, they are very different statuses. Rights granted to permanent residents include the right to work in Germany and access to welfare benefits. However, permanent residents do not have the right to vote in general elections, are unable to apply for civil servant positions, cannot work in other EU countries, may lose their residency status if they are out of Germany for more than a year, and face the risk of deportation if they commit a crime. It is well established in the literature that the two legal statuses, citizenship and residency, have different implications for immigrants' labor market outcomes, which we discuss below.

<sup>6</sup> The main reason for this was a lack of publicity. On the one hand, families were not directly informed about the transition rule. On the other hand, the public

**Table 1**  
**Access to Citizenship for Immigrant Children Born in Germany**

	Born $\leq$ 1999: Ius Sanguinis	Born $\geq$ 2000: Ius Soli
Principle	Citizenship inherited through parents	Citizenship automatically acquired by birth on German soil
Condition	Previous naturalization of at least one parent	Eight-year residency in Germany of at least one parent
Exception	Transition rule from January 1 to December 31, 2000	None
Implication	20% of children born in 1999 acquired citizenship at birth	72% of children born in 2000 acquired citizenship at birth

NOTE.—Own representation, based on Worbs (2014).

Children who acquired German citizenship via *ius soli* or the transition rule were allowed to hold two passports up to age 23. At that point, they were expected to officially declare whether they wanted to maintain their German citizenship or keep the citizenship of their parents (the so-called option model). The declarations made by children born in 1991 (who acquired German citizenship through the transition rule) were almost exclusively in favor of German citizenship (Worbs 2014).<sup>7</sup>

The last row of table 1 provides key information on the variation we will exploit. It shows that the regime switch resulted in a pronounced increase in the share of naturalized second-generation immigrant children, defined throughout as children born in Germany to foreign-born parents: among those born in 1999, approximately 20% acquired German citizenship via either *ius sanguinis* or the transition rule; among those born in 2000, 72% qualified for German citizenship at birth.<sup>8</sup> Because of the residency criterion,

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discussion mainly revolved around the eligibility criteria for adult naturalization and birthright citizenship, while the transition rule did not receive much attention.

<sup>7</sup> In 2014, the year when the cohort born in 1991 had to opt for or against keeping German citizenship, the option model was abolished. Since then, any immigrant child having been granted German citizenship via *ius soli* or the transition rule can remain a dual citizen as long he or she has resided in Germany for more than 8 years, attended a German school for at least 6 years, or acquired some formal education in Germany (see §29 of the German citizenship law).

<sup>8</sup> These numbers are based on data from the German microcensus (GMC). Specifically, we used the 2001 wave to obtain information on children's citizenship status and their parents' citizenship, country of birth, and length of residence. As such, we are able to define second-generation immigrant children and calculate both the share having acquired German citizenship via *ius sanguinis* or the transition rule and the share being eligible for automatic *ius soli*. Given our definition of second-generation immigrant children, which is common in the literature, this group may contain children of German nationals born abroad (among them ethnic Germans), a subgroup that qualified for citizenship at birth through *ius sanguinis*.

not all immigrant groups were equally affected by the introduction of *ius soli*. In section IV, we present evidence showing that disproportionately many children with Turkish ancestors, but disproportionately few with ancestors from the former Soviet Union and the Balkans, were eligible for birthright citizenship.

As mentioned above, the reform also changed the path to citizenship for adults: it relaxed the length-of-residence requirement from 15 years to 8. The costs and efforts associated with naturalization, however, remained high: when applying for naturalization, immigrant adults needed to exhibit sufficient language proficiency, declare loyalty to the German constitution, prove the nonreceipt of welfare benefits, and, most important, resign their original citizenship.<sup>9</sup>

### B. The German Education System

The German education system comprises three parts: (i) the phase between birth and primary school, which is divided into early care available for children up to age 2 and preschool available for children ages 3–5; (ii) primary school, which usually begins at age 6 and continues for 4 years; and (iii) secondary school, which typically begins at age 10 and continues for between 5 (compulsory) and 9 years.

Preschool attendance is noncompulsory in Germany. Nevertheless, since 1996 every child turning 3 years old has been legally entitled to a slot in preschool. As a result, the supply of preschool places rose dramatically in the late 1990s and ultimately met demand in the early 2000s. Preschool is heavily subsidized on a sliding income scale. In the event of severe financial constraints, fees can be even reimbursed by the local youth welfare service. In 2012, 96% of all 3–5-year-old native children attended preschool, while the corresponding share of immigrant children was 87% (Bildungsberichterstattung 2012).

Between birth and primary school, children undergo several compulsory medical screenings. These medical screenings serve to document children's health, diagnose medical anomalies, and provide necessary treatment as early as possible. An important medical screening is the SEE, which is offered by the local health service and takes place in the year prior to entering primary school. In particular, all children turning 6 years old between July of the year

<sup>9</sup> Given the general increase in naturalizations in the aftermath of the reform, the share of immigrant children living with naturalized parents may have increased. Our main comparison, however, relies on children born between July 1999 and June 2000 and thus on children who were at most 6 months old when the new law came into effect. As such, the difference between children born before and after the cutoff in terms of having a naturalized parent amounts to 6 months at most, and the difference in terms of having a parent with the clear intention to naturalize should be basically zero. For children belonging to our control group—born between July 1998 and June 1999—a similar logic applies.

prior to enrollment and June of the same year as enrollment are subject to the SEE.<sup>10</sup> In addition to documenting a child's health, pediatricians determine whether a child is prepared to follow the school curriculum. The school readiness diagnosis is an important factor in school enrollment: a negative assessment can lead to school entry being deferred by 1 year. However, the ultimate decision of when to enroll a child in primary school is in the hands of parents. Indeed, in Germany parents are free to enroll a child for primary school either earlier or later than scheduled.

After passing all four grades of primary school, students are referred to secondary school (around age 10). Secondary school is divided into the following three tracks: the lowest track (the so-called *Hauptschule*), which continues up to grade 9 and gives students a general education in preparation for an apprenticeship; the intermediate track (*Realschule*), which goes up to grade 10 and can either lead to an apprenticeship or to a higher-level vocational school; and the academic track (*Gymnasium*), which goes up to grade 12 or 13 and prepares students for university.<sup>11</sup>

The transition from primary school to the different secondary school tracks is a critical stage in the German education system. In the final year of primary school, teachers make recommendations on which secondary school track students should enter. These recommendations are supposed to reflect students' abilities only. During our study period, the ultimate track choice was at parental discretion in 10 of 16 German federal states, including Schleswig-Holstein.<sup>12</sup> In 2009, 25.9% of all 15-year-old immigrant students attended Gymnasium, in comparison to 37.1% of their native peers (Bildungsberichterstattung 2012). In other words, the unconditional immigrant-native enrollment gap in the academic track amounted to 11.2 percentage points. Lüdemann and Schwerdt (2013) show that one-half of this gap remains after accounting for children's performance during primary school, which might be due to discrimination by teachers, lower levels of effort, motivation or perseverance among immigrant students, or lower educational aspirations among immigrant families (or a combination thereof).

<sup>10</sup> In Germany, the cutoff date for school entry was June 30 throughout most of the twentieth century. Since 1997, German states have been allowed to freely set their own cutoff dates, and several states have independently postponed it: to August 1 (Thuringia), September 30 (Baden-Württemberg, Bavaria, Brandenburg, Lower Saxony, and North-Rhine Westfalia), and December 31 (Berlin). However, in Schleswig-Holstein, which is the context of our study, the cutoff date remained June 30.

<sup>11</sup> Approximately 10% of all children of secondary school age attend alternative school types, such as Waldorf schools and comprehensive school (*Gesamtschule*).

<sup>12</sup> In the remaining states, a child either had to pass a special exam or undergo a probationary period if he or she wished to attend Gymnasium without a recommendation.

### C. Potential Mechanisms

At each stage of the education system, families have to make choices about their children's educational career. At earlier stages, parents are the sole decision-makers, while at later stages children are likely involved in the process. For instance, parents decide whether to send their child to preschool and when to enroll their child in primary school. In the transition from primary to secondary school, parents and children may decide together which school track is best for the child.

Endowing immigrant children with citizenship rights at birth is an early-life intervention likely to affect these choices. First, immigrant parents may perceive birthright citizenship as a "sign of goodwill" by the host country and may therefore be motivated to integrate. Indeed, exploiting the same reform as we do, Avitabile, Clots-Figueras, and Masella (2013) and Sajons (2012) provide evidence that foreign-born parents are more likely to speak the German language and to interact with the local community if their children are endowed with citizenship rights.<sup>13</sup> As a consequence, they may be better informed about the local education system and make educational choices similar to those made by native parents.

Second and most important from our point of view, citizenship improves immigrants' professional opportunities. In Germany, it opens the door to any job requiring civil servant status, entitles individuals to work in any EU country, and allows visa-free entry to many other countries.<sup>14</sup> It has been argued that citizenship may act as signal to employers that the prospective employee is committed to stay and integrate into the host society. The existing evidence suggests that naturalized immigrants, compared with their nonnaturalized peers, earn more (Chiswick 1978; Steinhardt 2012), have higher job-finding rates (Fougère and Safi 2009; Gathmann and Keller 2018), and experience steeper wage-tenure profiles (Bratsberg, Ragan, and Nasir 2002). Thus, it can be hypothesized that being endowed with citizenship at birth represents a positive shock to the long-run rate of return to education for immigrant children. This, in turn, may trigger human capital investments by parents that foster their children's educational integration. It may also affect immigrants' return migration decision (Sajons 2016), which has been shown to have a large impact on their human capital investments (Dustmann 1999). Finally, a positive shock to the rate of return to education can be seen as a reduction in the price of child quality. In line with the predictions of the quantity-quality

<sup>13</sup> It could also be that citizenship rights for newborns induce parents to naturalize. For the reform under study, Avitabile, Clots-Figueras, and Masella (2013) and Sajons (2012) have shown this not to be the case.

<sup>14</sup> There are 1.4 million civil servant posts in Germany, which correspond to 4% of all jobs in Germany. Civil servant posts are either prestigious (e.g., teachers or judges) or include jobs that might appeal to children (e.g., firefighters or police officers).

model (Becker and Tomes 1976), Avitabile, Clots-Figueras, and Masella (2014) show that the introduction of birthright citizenship led to a reduction in immigrant fertility and had positive short-run effects on immigrant children's behavioral and health outcomes, evidence of increased parental investments.<sup>15</sup>

Thus far, it is unknown whether the hypothesized effect of birthright citizenship on immigrant children's education exists. Of particular interest is whether this effect (if any) persists in the long run. It is well understood that educational choices are hierarchical, with those made early in a child's educational career affecting subsequent choices (Cunha and Heckman 2007). Preschool enrollment may promote a child's school readiness (Cornelissen et al. 2018) and thus influence parents' decisions regarding primary school enrollment. Age at school entry, in turn, may affect children's subsequent scholastic performance and thus secondary school track choice. In light of this hierarchical structure, two scenarios for the effects of birthright citizenship are conceivable. On the one hand, parental choices regarding their children's early education—for example, sending them to preschool—may result in improved child outcomes in the short run that in turn trigger subsequent parental investments that promote children's education in the long run. On the other hand, returns to investments may not meet parents' expectations (e.g., in the form of improved teacher evaluations). If this is the case, parental integration efforts may fade out over time, and no lasting effect on children's education would be observed.

### III. Empirical Approach

#### A. Identification Strategy

Our identification strategy rests on a difference-in-differences design that exploits the cutoff date of the German citizenship reform (i.e., January 1, 2000). In particular, we compare immigrant children who were born shortly before and shortly after the cutoff date. To avoid differences across school cohorts, we restrict our attention to one school cohort; that is, we compare children born 6 months before and after the cutoff date, thus, between July 1999 and June 2000. A key concern with this simple difference is that the characteristics of children and parents might systematically vary on the two sides of the cutoff date. First, children belonging to one school cohort differ by up to 12 months in age, with children born in the earlier months being likely to be more mature at any given point in time. In addition, there is

<sup>15</sup> We investigated the reform effect on the number of younger siblings. In line with the argument advanced by Avitabile, Clots-Figueras, and Masella (2014), we found a negative impact on the number of younger siblings. The results are available on request.

evidence that children born in spring come from more advantaged socioeconomic backgrounds than children born in winter (Buckles and Hungerman 2013). To avoid biased estimates due to such age and seasonal effects, we use immigrant children from the preceding school cohort as a control group. In particular, our control group comprises immigrant children born in the 12-month window centered around January 1, 1999, thus, between July 1998 and June 1999.<sup>16</sup> Notice that these children were born under the old *ius sanguinis* regime, and no policy change occurred at this control cutoff date. Our regression model can be written in the following way:

$$Y_{i,m} = \alpha + \beta Treated\ Cohort_{i,m} + \gamma After_{i,m} + \delta (Treated\ Cohort_{i,m} \times After_{i,m}) + \sum_m \theta_m D_{i,m} + \varepsilon_{i,m}, \quad (1)$$

where *Treated Cohort*<sub>*i,m*</sub> is a binary variable indicating whether child *i* born in month *m* belongs to the school cohort that was subject to the policy change (i.e., it switches on for children born between July 1999 and June 2000). The coefficient  $\beta$  captures any differences between immigrant children born in the treated and the control cohort. *After*<sub>*i,m*</sub> is a binary assignment variable indicating whether child *i* was born in the months just after January 1 of a given school cohort (i.e., it switches on for children born between January and June). The coefficient  $\gamma$  captures differences (e.g., due to age or seasonal effects) between children born in the different semesters of a school cohort. To allow for any heterogeneity that may arise because children are born in different months, we further control for a full set of month-of-birth dummies  $D_{i,m}$ .<sup>17</sup> Finally, we include the interaction between *Treated Cohort*<sub>*i,m*</sub> and *After*<sub>*i,m*</sub>, which switches on for children born between January and June 2000, thus, for all children born after the policy change introducing the *ius soli* regime. The corresponding coefficient  $\delta$  is our parameter of interest, and we discuss its interpretation in detail in the following subsection. We estimate equation (1) separately for various educational outcomes. Standard errors  $\varepsilon_{i,m}$  are clustered at the birth-month/year level.

<sup>16</sup> Similar identification strategies have been used by Lalive and Zweimüller (2009), Dustmann and Schönberg (2012), Danzer and Lavy (2018), and Schönberg and Ludsteck (2014) in the context of parental leave reforms.

<sup>17</sup> The assignment variable *After*<sub>*i,m*</sub> correlates perfectly with the birth months January to June. We therefore omit not just one but two birth-month dummies. Specifically, we omit January and December, as they are immediately around the cutoff date. Note that we can control for the set of birth-month dummies because we rely on a comparison between children born in the year of the policy change and children born in the preceding year, in which there was no policy change. A simple regression discontinuity design would not allow us to do so, as the assignment variable would be a perfect linear combination of the included set of birth-month dummies.

### B. Interpretation of the Parameter of Interest

If we estimate equation (1) on a sample of eligible immigrant children, the coefficient of interest  $\delta$  represents the reduced-form effect of the switch from *ius sanguinis* to *ius soli*. It can be interpreted as the intention-to-treat (ITT) effect of granting second-generation immigrant children citizenship rights at birth. This interpretation is complicated by two features of our main data sources. First, they do not contain information on the length of parental residence, and so our estimation samples will cover both children who are eligible and children who are ineligible for *ius soli*. As a result, our estimates of  $\delta$  likely underestimate the ITT effect: ineligible children did not obtain citizenship rights at birth, and hence there is no direct impact on their educational outcomes. This is a setting with classical measurement error, which leads to attenuation bias. It is possible that there are spillover effects if, for example, ineligible families have frequent contact with eligible families and consequently adjust their educational choices. However, these spillover effects (if any) unlikely exceed the direct reform effects. Thus, our estimates should represent conservative estimates of the ITT effect.<sup>18</sup> Second, our main data sources do not allow for a common definition of a child's migrant status, and we discuss the consequences of this in greater detail in section IV.

The estimates of  $\delta$  would identify the average treatment effect of endowing immigrant children with citizenship at birth if the birth date cutoff perfectly determined whether a child obtained German citizenship. Since this is not the case, one is left with the possibility of exploiting the birth date cutoff as a source of exogenous variation in a child's citizenship at birth and to employ an instrumental variable approach. However, limitations of our data—that is, the lack of information on children's citizenship status at birth—prevent us from adopting such an approach. Data limitations are also the reason that we cannot pursue a proper two-sample instrumental variable approach.<sup>19</sup> Nevertheless, we are able to provide estimates of the treatment intensity around the cutoff date. As shown in table 1, 20% of second-generation immigrant children born in 1999 received German citizenship via *ius sanguinis* or the transition rule; in contrast, 72% of the 2000 birth cohort automatically qualified for German citizenship via conditional *ius soli*. Thus, the treatment intensity for second-generation immigrant children, both eligible and ineligible, corresponds to 52 percentage points. We therefore need to scale the estimates of  $\delta$  by a factor of 1.92 to derive back-of-the-envelope estimates of the average treatment effect of endowing immigrant children with citizenship at birth. In section IV.C, we discuss the extent to which our two main data sources require us to further adjust this scaling factor.

<sup>18</sup> Table A3, based on auxiliary data from the GMC and discussed in greater detail below, provides evidence in support of this argument.

<sup>19</sup> Survey data offering information on children's citizenship status do not offer a sufficient number of observations to conduct any analysis at the state level.

### C. Threats to Identification

There are several potential threats to identification. The major concern is that the reform had a direct effect on the composition of children born shortly before and shortly after the reform. While descriptive evidence speaks against this concern (see sec. IV, specifically table 2, for details), in what follows we discuss the reasons for potential compositional changes and how we address such threats.

#### *Fertility Behavior*

The German citizenship reform may have induced immigrant parents to adjust their fertility behavior in three dimensions. First, parents may have delayed conception to ensure that their child is born under the new *ius soli* regime. As a result, we may observe a spike in the number of children born to immigrants around the cutoff date. Closer inspection, however, does not reveal any discontinuity in the number of immigrant children born around the cutoff date (see fig. A1). Second, as mentioned above, the introduction of birthright citizenship can be seen as a reduction in the price of child quality, which may have caused immigrant parents to adjust their desired number of children downward. Avitabile, Clots-Figueras, and Masella (2014) provide evidence that the German citizenship reform indeed led to a reduction in immigrant fertility but only from 2001 onward. Hence, children included in our sample, who were conceived before September 1999, are unlikely to be affected by this concern. Notwithstanding this, we test the robustness of our results to restricting our sample to children who were conceived before the new German naturalization law was ratified, that is, those conceived before July 1999 and thus born by April 2000. This leaves us with an 8-month window centered on the cutoff date. Third, in addition to manipulating the date of conception, mothers scheduled to give birth shortly before the cutoff date may have attempted to postpone birth to benefit from *ius soli*. Although this type of behavior is difficult to implement, we perform a “donut” check in which we exclude children born in the months immediately before and after the cutoff date (i.e., December 1999 and January 2000).

#### *Return Migration*

A second concern is that the introduction of birthright citizenship made return migration less attractive. As a result, the pool of families remaining in the country may have changed after the introduction of birthright citizenship. As mentioned above and discussed further in section IV, we do not observe any significant differences in the available background characteristics between immigrant children born shortly before and after the cutoff date. Hence, our estimates are robust to extending our baseline specification and including available family background characteristics, such as single parenthood,

**Table 2**  
**Summary Statistics: School Entry Examination**

	Prereform		Balancing for Migrants	
	Natives II/99	Migrants II/99	Gap	Treated I/00-II/99
A. Dependent Variables: Preschool				
Preschool enrollment	.950	.917	-.033***	.025**
School readiness	.919	.863	-.055***	-.091***
Socioemotional maturity	.890	.902	.012	.001
Language proficiency		.580		-.077***
B. Background Variables				
Age in months	76.234	76.396	.162*	-.5672***
Female	.462	.468	.006	.040
Number of siblings	.902	1.287	.385***	-.030
Single parent	.144	.088	-.055***	-.001
Mom's education: low	.196	.220	.025*	.014
Mom's education: intermediate	.335	.240	-.095***	.000
Mom's education: high	.222	.154	-.068***	.015
Mom's education: missing	.247	.385	.138	-.030
Dad's education: low	.232	.236	.004	-.014
Dad's education: intermediate	.228	.224	-.004	.021
Dad's education: high	.246	.154	-.092***	.027
Dad's education: missing	.294	.386	.092	-.024
Mom's origin: Turkey		.306		.009
Mom's origin: Eastern Europe		.405		-.011
Mom's origin: Balkan		.087		-.018
Mom's origin: EU 12		.040		.015
Mom's origin: other		.157		.005
Dad's origin: Turkey		.311		.005
Dad's origin: Eastern Europe		.393		-.016
Dad's origin: Balkan		.091		-.024
Dad's origin: EU 12		.042		.009
Dad's origin: other		.156		.025

NOTE.—“Prereform” refers to children born between July and December 1999. “Migrants” refers to children whose parents were both not born in Germany, whereas “Natives” refers to children whose parents were both born in Germany. “Prereform Gap” refers to the gap between migrants and natives born before the reform. The last two columns (“Balancing”) report the results of balancing tests regarding migrant children born in the 6-month window before and after the cutoff date (January 1) in the treated cohort (July 1999 and June 2000) and the control cohort (July 1998 and June 1999).

\* 10% significance level.

\*\* 5% significance level.

\*\*\* 1% significance level.

parental education, and parents' country of origin.<sup>20</sup> However, differences in unobservable background characteristics may still be a threat to identification. To address this concern, we provide a bounds analysis in which we boost the prereform samples by 10% (Sajons 2016) and assume two extreme scenarios: a pessimistic scenario in which all added children represent the "worst case" (i.e., did not attend preschool, repeated a grade, and did not attend Gymnasium), and an optimistic scenario in which they perform as well as the average native child.

#### *Miscellaneous*

Finally, we address potential concerns regarding remaining differences between children born in the different semesters of a school cohort that may not be captured by the preceding cohort by adding yet another school cohort to the control group. Specifically, we draw on the adjacent post-reform cohort, children belonging to the cohort born in the 12-month window around January 1, 2001, and control flexibly for cohort dummies and their interactions with birth semester.

### IV. Data

The questions we ask, and how we answer them empirically, require very comprehensive data. First, we need information on immigrant children throughout the first three stages of the German education system. Second, our identification strategy relies on a very small subgroup of the German population: a few cohorts of immigrant children. We therefore draw on two unique administrative data sources from the German federal state of Schleswig-Holstein: SEEs and SRs. Both data sources allow us to proxy for children's immigrant status, but they differ in the information available to do so. Moreover, they both lack information on parents' length of residence and, hence, do not allow us to restrict the samples to eligible immigrant children. At the end of this section, we will therefore discuss the comparability of the two data sources with a baseline sample of second-generation immigrant children and the consequences thereof for our estimates.

#### A. School Entry Examination

Administrative records from SEEs contain data on children's educational outcomes during the preschool phase. This examination is compulsory for all children who turn 6 in the 12-month period before a new school year begins. Children born between July 1998 and June 2000 are thus included in the SEE 2005 and 2006.

<sup>20</sup> Notice that this is only possible in one of the data sets used in this study (see sec. IV for further details).

Pediatricians assess children's development in various dimensions, including language proficiency, socioemotional maturity, and overall school readiness. Parents complete an accompanying questionnaire that asks them to provide information about, *inter alia*, children's preschool enrollment and family background.<sup>21</sup> For the latter, we have information about parents' country of birth but not about their length of residence in Germany. Moreover, all we know is whether a child's family was living in Germany at the time of the survey, not whether the child was born in Germany. For our estimation sample, we restrict attention to children whose parents were both not born in Germany, which leaves us 4,450 observations. The SEE records allow us to construct the following binary dependent variables:<sup>22</sup>

- *Preschool Enrollment* is equal to 1 for children who attend preschool for at least 1 year and 0 otherwise (available for all counties; no missing data).
- *Language Proficiency* is equal to 1 for children who are fluent in German or make at most small mistakes and 0 otherwise (available in 2005 for only 1 of 15 counties and in 2006 for 11 of 15 counties; missing for 72% of the sample).<sup>23</sup>
- *Socioemotional Maturity* is a measure combining several developmental deficits—attention deficit, emotional instability, antisocial and deviant behavior—and is equal to 1 if none of the mentioned deficits is diagnosed and 0 otherwise (available in 11 of 15 counties; missing for 22%).
- *School Readiness* is equal to 1 for children assessed to be ready for primary school and 0 otherwise (available for all counties; no missing data).

Table 2 displays descriptive statistics from the SEE records. The first three columns, which describe and compare native and immigrant children born prior to the reform, reveal significant immigrant-native gaps in several early education outcomes. The preschool enrollment rate of immigrant children is significantly lower than that of their native peers (91.7% vs. 95.0%). Moreover, while 91.9% of native children are assessed to be ready

<sup>21</sup> The questionnaire does not contain information on children's citizenship. As such, pediatricians cannot discriminate on this dimension.

<sup>22</sup> Some of them may not be available for all counties of Schleswig-Holstein because they were either not surveyed or not transferred to the central authority in charge of data collection.

<sup>23</sup> The county Hansestadt Lübeck was the first county to assess immigrant children's German language proficiency in 2005. In 2006, 10 further counties followed. As such, our control cohort contains children from a single county only. We address any resulting concern via a robustness check that draws additionally on data from the SEE 2007.

to enter primary school, the corresponding share among immigrant children amounts to only 86.3%. There is no significant difference between immigrant and native children in terms of their socioemotional maturity.

The fourth column of table 2 provides balancing tests for immigrant children belonging to the treated cohort; it compares early education outcomes (panel A) and background characteristics (panel B) of children born 6 months before and after January 1, 2000. The fifth column provides the same comparison for immigrant children belonging to the control cohort, that is, those born in the  $\pm 6$ -month window around January 1, 1999. In the treated cohort, immigrant children born after the policy are 2.5 percentage points more likely to be enrolled in preschool than those born before the policy. In the control cohort, we do not observe a significant difference. Next, among children belonging to the treated cohort, those born before the policy are 9.1 percentage points less likely to be assessed school ready than those born after the policy. This difference is likely driven by age differences between pre- and post-policy children (5.7 months on average; see panel B, col. 4). Indeed, in the control cohort the school readiness gap between children born before and after January 1, 1999, is even more pronounced (11.7 percentage points). Comparing immigrant children in the treated cohort in terms of language proficiency, we find that 58% of those born before January 1, 2000, are assessed to be fluent in German, in comparison to 50.3% of those born after the cutoff date, a gap of 7.7 percentage points. In the case of the control cohort, this gap amounts to 11.8 percentage points. In terms of socioemotional maturity, there is no significant difference between children born before and after the policy in the treated cohort. However, in the control cohort children born after January 1, 1999, are 2.8 percentage points less likely to be assessed as socioemotionally mature than those born before it. Importantly, aside from age gaps of 5.7 months in the treated cohort and 5.9 months in the control cohort, there are no significant demographic or socioeconomic differences between immigrant children born shortly before and after the respective cutoff date (see panel B). This supports the assumption of comparability between the children born before and after the cutoff dates.

## B. School Registers

SR data provide us with information on educational outcomes during the primary and secondary school phase. In Schleswig-Holstein, all primary and secondary schools are legally obliged to provide individual student records to the federal ministry of education at the beginning of each school year (i.e., September). We draw on the records from 2009 and 2010.

The SR data provide basic information about children's gender and birth date. In addition, they include two variables that allow us to proxy for children's migrant status: their country of birth and main language spoken at home. We restrict our sample to children who are born in Germany but

do not use German as their main language at home. The language restriction implies that a substantial share of immigrant children are excluded from our SR sample. In fact, despite being drawn from the same population, the sample based on the 2009 and 2010 SR records contains substantially fewer observations than the sample based on the 2005 and 2006 SEE records (1,626 vs. 4,450 observations). The SR data allow us to construct the following dependent variables for the primary school phase:

- *Age at School Entry* is a continuous variable measuring children’s age (in months) at primary school entry (available for all children; no missing data).
- *Early School Entry* is equal to 1 for children who enter primary school earlier than scheduled and 0 otherwise (available for all children; no missing data).
- *Grade Retention* is equal to 1 for children who repeat one or more grades during primary school and 0 otherwise (available for all children; no missing data).

As for secondary school education, our intention is to assess children’s tracking outcomes at the beginning of secondary school. By using SR records from 2009 and 2010, we are able to do this for children who had begun secondary school on schedule. However, our sample does not include “late-comers” who entered primary school with a delay or had to repeat a grade (19% of the school cohorts under study). In a sensitivity check, we will present estimates based on SR records from 2012 and 2013. By that time, the cohorts we study were scheduled to attend eighth grade, and all children—that is, both those with regular progress as well as latecomers—should have been tracked into secondary school. Moreover, the sensitivity check will show whether any potential reform effect on children’s tracking outcomes at the beginning of secondary school persists at higher grades.<sup>24</sup> We construct the following dependent variables for secondary school education:

- *Recommendation* is equal to 1 for children who receive the official recommendation to attend the academic track of secondary school and 0 otherwise (available for children attending secondary school on schedule; 19% missing).
- *Gymnasium* is equal to 1 for children who are enrolled in the academic track of secondary school and 0 otherwise (available for children attending secondary school on schedule; 19% missing).

<sup>24</sup> Ideally, we would also like run this check for the last grade of mandatory schooling (i.e., ninth grade). However, the SR records for 2014 do not provide any information on track recommendations, and we therefore abstain from using them in the analysis.

- *Gymnasium with Recommendation* is equal to 1 for children who receive the official recommendation and attend the academic track of secondary school and 0 otherwise (available for children attending secondary school on schedule and set equal to missing if enrolled in the academic track without recommendation; 29% missing).
- *Gymnasium without Recommendation* is equal to 1 for children who do not receive the official recommendation but nevertheless attend the academic track of secondary school and 0 otherwise (available for children attending secondary school on schedule and set equal to missing if enrolled in the academic track with recommendation; 28% missing).

Table 3 presents descriptive statistics from the SR records. The first three columns describe and compare native and immigrant children (i.e., who do and do not speak German at home) born prior to the reform. Compared with their native peers, immigrant children are significantly less likely to enter primary school ahead of the scheduled year of admission (8.7% vs. 14.7%), more likely to be retained (23.7% vs. 13.4%), and, conditional on

**Table 3**  
**Summary Statistics: School Register Data**

	Prereform			Balancing for Migrants	
	Natives II/99	Migrants II/99	Gap	Treated I/00-II/99	Control I/99-II/98
A. Dependent Variables: Primary School					
Age at school entry	81.925	82.915	.990***	-4.917***	-3.305***
Early school entry	.147	.087	-.060***	-.066***	-.178***
Grade retention	.134	.237	.103***	.073**	.126***
B. Dependent Variables: Secondary School					
Recommendation	.406	.151	-.255***	-.050**	-.092***
Gymnasium	.442	.244	-.198***	-.054**	-.147***
With Recommendation	.389	.152	-.237***	-.045**	-.115***
Without Recommendation	.121	.125	.004	-.024	-.079***
C. Background Variables					
Age in months	131.576	131.616	-.042	-5.820***	-5.804***
Female	.488	.493	.005	.025	.004

NOTE.—“Prereform” refers to children born between July and December 1999. “Migrants” refers to children who do not use German as their main language at home, whereas “Natives” refers to children who use German as their main language at home. “Prereform Gap” refers to the gap between migrants and natives born before the reform. The two last columns (“Balancing”) report the results of balancing tests regarding migrant children born in the 6-month window before and after the cutoff date (January 1) in the treated cohort (July 1999 and June 2000) and the control cohort (July 1998 and June 1999).

\*\* 5% significance level.

\*\*\* 1% significance level.

having progressed through primary school on schedule, less likely to receive a recommendation for the academic track of secondary school (15.1% vs. 40.6%) as well as to actually attend this track (24.4% vs. 44.2%).

The fourth column of table 3 provides balancing tests for immigrant children belonging to the treated cohort; that is, it compares children born 6 months before and after January 1, 2000. Panel A shows that children born after the cutoff date are less likely to start school ahead of the scheduled year of admission (by 6.6 percentage points), are more likely to repeat a grade during primary school (by 7.3 percentage points), are less likely to receive a positive recommendation for the academic track of secondary school (by 5.0 percentage points) and are also less likely to attend the academic track of secondary school (by 5.4 percentage points). As previously discussed and confirmed by the data for the control cohort (see col. 5), these differences are likely driven by the age difference between children born before and after the cutoff date (5.8 months for children belonging to both the treated and the control cohort). This underlines the importance of using a difference-in-differences approach to net out potential age and seasonal effects. The SR does not provide detailed background information and thus prevents us from conducting balancing tests in this dimension.<sup>25</sup>

### C. Sample Issues

In section III.B, we discussed the interpretation of our estimates for a baseline sample composed of second-generation immigrant children, that is, those born in Germany to foreign-born parents. Our two estimation samples differ from this baseline. In the SEE records, which do not contain information on children's country of birth, immigrant children are defined as those born to foreign-born parents. In the SR records, which do not contain information on parents' country of birth, immigrant children are defined as those born in Germany who speak a language other than German at home. We now discuss the extent to which our two estimation samples are comparable and the consequences of any differences for our estimates.

In a first step, we draw on the NEPS. Specifically, we rely on the cohort starting in fifth grade, which predominantly contains children born between July 1999 and June 2000. Using information on children's and parents' country of birth, we construct the baseline sample of second-generation immigrant children. Relaxing the restriction regarding children's country of birth results in a sample analogue to the SEE sample. Focusing on German-born children whose main language at home is not German yields a sample

<sup>25</sup> An analysis based on the GMC (waves 2010 and 2011) does not reveal any significant differences between children born before and after the cutoff date, except for the previously discussed differences in children's age and number of siblings (see table A2, col. 4).

analogue to the SR sample. Table 4 displays descriptive statistics for all three samples.

Let us first compare the SEE sample analogue to the baseline. By definition, the SEE sample analogue contains both first- and second-generation immigrant children. Specifically, 15% of all children in the SEE sample analogue are born abroad and only move to Germany at some point before turning 6 years old. This difference is also reflected in the language spoken at home: compared with the baseline, the SEE sample analogue contains 13 percentage points more children who do not speak German at home. Notwithstanding this, the SEE sample analogue and the baseline sample are comparable in terms of all available background characteristics. There are no major differences in the distribution of parents' countries of origin. There is also no difference between the two samples in terms of families' socioeconomic status. Recall that in the baseline sample, 20% of immigrant children born before the policy became German citizens at birth through ius sanguinis or the transition rule, while 72% of those born after the regime switch acquired German citizenship at birth. In the SEE sample, these shares should be 15% lower (17% vs. 61%): prepolicy children born abroad

**Table 4**  
**Sample Comparison Based on the National Education Panel Study (NEPS)**

	Baseline	SEE Sample Analogue	SR Sample Analogue
A. Sample Restrictions			
Born in Germany	1.00	.85	1.00
Main language is German	.74	.61	.00
B. Family Background			
Mom is born in Turkey	.31	.27	.27
Mom is born in the Soviet Union	.27	.31	.15
Mom is born in Poland	.09	.09	.05
Mom is born in the Balkans	.07	.07	.07
Mom is born in the EU	.04	.04	.04
Mom is born in Germany	.00	.00	.17
Dad is born in Turkey	.31	.27	.28
Dad is born in the Soviet Union	.25	.30	.15
Dad is born in Poland	.09	.09	.05
Dad is born in the Balkans	.07	.06	.08
Dad is born in the EU	.05	.05	.05
Dad is born in Germany	.00	.00	.12
Socioeconomic status	.76	.76	.75
N	385	448	197

NOTE.—Own calculations based on the NEPS school cohort 3 (fifth grade) interviewed in 2010 (<https://doi.org/10.5157/NEPS:SC3:1.0.0>; see also Blossfeld, Roßbach, and Maurice 2011). The baseline sample includes all children born in Germany to foreign-born parents. The school entry examination (SEE) sample analogue further includes children of foreign-born parents who are not born in Germany. The school register (SR) sample analogue consists of children born in Germany who speak a language other than German at home. The socioeconomic status score is a weighted average of the availability of certain items at home, e.g., own desk, own room, own books, and art work at home.

are unlikely to have qualified for citizenship through *ius sanguinis*, and those born after the policy did not qualify for *ius soli* by definition. This implies that the treatment intensity corresponds approximately to 44 percentage points. Thus, the back-of-the-envelope scaling factor for the average treatment effect of citizenship at birth amounts to 2.3.

Consider next the SR sample analogue. In addition to language spoken at home, the key difference from the baseline and SEE sample analogue is that a nonnegligible share of children are third-generation immigrants, that is, children born to German-born parents. Specifically, among all immigrant children included in the SR sample analogue, 21% have at least one parent who was born in Germany (17% of the mothers; 12% of the fathers). Further inspection reveals that these families have predominantly Turkish or Eastern European origins: 58% use Turkish as the main language at home, and for 17% the main language at home is Albanian, Croatian, Serbian, or Polish. Compared with the baseline and SEE sample analogue, the SR sample analogue also contains fewer children of parents born in the Soviet Union. There are no major differences in terms of parents' socioeconomic status. To obtain an estimate of the treatment intensity for the SR sample analogue, some assumptions regarding third-generation immigrant children are necessary. For those born after the policy, it seems reasonable to assume that parents who were born in Germany fulfilled the eligibility criterion of at least 8 years of legal residence in Germany. Under this assumption, the share of children born after the reform who qualified for *ius soli* corresponds to 78%.<sup>26</sup> Regarding children born before the policy, we consider two scenarios. First, suppose that the share of parents who had naturalized prior to their children's birth is the same for parents born in and outside Germany. In this case, the share of children born before the reform and qualifying for citizenship based on *ius sanguinis* amounts to 20%. Second, suppose that all German-born parents were naturalized before their child was born. In this case, 37% of all children born before the reform qualify for *ius sanguinis*.<sup>27</sup> Thus, the treatment intensity lies between 40 and 57 percentage points, and the back-of-the-envelope scaling factor for the average treatment effect of citizenship at birth is between 1.8 and 2.5.

In summary, the evidence from the NEPS suggests that the SEE sample differs from the SR sample along two dimensions. First, 40% of children in the SEE sample speak a language other than German at home, while in the SR sample this share amounts to 100%, by definition. Second, unlike the

<sup>26</sup> As in the baseline sample, 72% of all second-generation immigrant children in the SR sample are assumed to qualify for *ius soli*, while the remaining third-generation immigrant children are assumed to be all eligible for *ius soli*. Thus, the share qualifying for *ius soli* amounts to  $79\% \times 72\% + 21\% \times 100\% = 78\%$ .

<sup>27</sup> Using the same logic as above, the share qualifying for *ius sanguinis* in this case is given by  $79\% \times 20\% + 21\% \times 100\% = 37\%$ .

SEE sample, the SR sample contains roughly 20% third-generation immigrant children, many of whom have Turkish or Eastern European backgrounds. Despite these differences, parents' socioeconomic status does not differ across the two samples.<sup>28</sup> The treatment intensity around the reform cutoff date, and thus the scaling factor for the average treatment effect, is comparable in the two samples.

Finally, we provide some descriptives regarding the composition of immigrant children eligible for birthright citizenship. For this purpose, we draw on the GMC, which contains information on parents' year of arrival in Germany. Table A2 compares the average second-generation immigrant child with the average eligible one. The only notable difference relates to parents' country of origin: disproportionately many children with Turkish ancestors, but disproportionately few children with ancestors from the former Soviet Union and the Balkans, were eligible for birthright citizenship. This aligns well with the different immigration waves that Germany experienced in the twentieth century: immigration from Turkey occurred mostly during the 1960s, while immigration from the former Soviet Union and the Balkans occurred predominantly in the mid- and late 1990s.

## V. Results

### A. Preschool

Table 5 presents our results for the reform effects at the preschool level. Column 1 controls only for the full set of month-of-birth dummies, while columns 2 and 3 sequentially condition on child and family background characteristics. In line with our assumption of random sorting around the cutoff, the estimates are robust to augmenting the set of control variables. In what follows, we discuss the results of our preferred specification in column 3.

The estimates reported in panel A show that the regime switch led to a 3.1 percentage point increase in the preschool enrollment rate of immigrant children. Thus, the share of immigrant children not enrolled in preschool (8.3% among those born shortly before the cutoff date) decreased by 37%. As a consequence, the preschool enrollment gap between immigrant children and their native peers, of whom 95.4% were enrolled in preschool, almost closed.

Parental investments early in life and preschool exposure are important inputs into children's human capital production function. In fact, it is well understood that preschool participation exerts positive effects on children's

<sup>28</sup> The observation that parents' socioeconomic status does not differ across the SEE and SR sample analogues is confirmed when using PISA data (see table A1). This data set has the advantage over the NEPS that it contains detailed information on parents' education. However, it has the disadvantage that it only samples children born before the policy.

**Table 5**  
**Reform Effects at the Preschool Level**

	(1)	(2)	(3)
A. Participation in the Early Education System			
Preschool enrollment [baseline = .917]	.034*** (.012)	.033*** (.011)	.031*** (.010)
N	4,450	4,450	4,450
B. Assessed Outcomes			
German language proficiency [baseline = .580]	.029* (.015)	.028* (.014)	.035** (.013)
N	1,248	1,248	1,248
Socioemotional maturity [baseline = .902]	.029*** (.010)	.021** (.011)	.019* (.011)
N	3,441	3,441	3,441
C. Recommendation			
School readiness [baseline = .863]	.024* (.014)	.025 (.016)	.022 (.017)
N	4,540	4,540	4,540
Birth months	Yes	Yes	Yes
Child characteristics		Yes	Yes
Family characteristics			Yes

NOTE.—Ordinary least squares estimates of eq. (1) using the school entry examination (SEE) records 2005 and 2006. Mean of dependent variable for children born between July and December 1999 reported in brackets. Child characteristics include gender and age at the SEE. Family characteristics include number of siblings, a dummy for single-parent household, parents' educational degree, and parents' country of origin. Standard errors are clustered at the birth-month/year level.

\* 10% significance level.

\*\* 5% significance level.

\*\*\* 1% significance level.

language, motor, and social skills, particularly among children at risk for poor outcomes (see, e.g., Cornelissen et al. 2018). In panel B, we therefore examine the reform effect on immigrant children's proficiency in German and on their socioemotional maturity as assessed at the end of the preschool phase. The results suggest positive and significant effects on immigrant children's language proficiency: the share speaking fluent German increases by 3.5 percentage points, from 58.0% to 61.5%. In line with the results of Avitabile, Clots-Figueras, and Masella (2014), we also observe an improvement in immigrant children's socioemotional development by 1.9 percentage points, which corresponds to a 2.1% increase from the prereform level. Further inspection reveals that the improvement in children's socioemotional maturity is driven primarily by a reduction in emotional problems and less so by a reduction in social deficits.

As part of the SEE, pediatricians provide an overall assessment of children's school readiness and may recommend deferred school entry. The results in panel C indicate that the reform had a positive, albeit nonsignificant,

effect on children's school readiness. In terms of magnitude, the estimate of school readiness is almost identical to that of socioemotional maturity, with a similar baseline mean. Taken at face value, it corresponds to a reduction in the immigrant-native school readiness gap by almost one-quarter (24.2%). That said, the result needs to be interpreted cautiously, since the estimate lacks precision.<sup>29</sup>

### B. Primary School

Table 6 documents the reform effects at the primary school level. In what follows, we discuss the results of our preferred specification in column 2, which controls for the full set of month-of-birth dummies and children's gender.

The key message that emerges from the first row in panel A is that the introduction of *ius soli* led to a reduction in immigrant children's school starting age by 1.6 months. As shown in the second row of panel A, this effect is driven by an increased incidence of early school entry: after the reform, the share of immigrant children starting primary school at age 5 instead of age 6 increases by 10.8 percentage points—that is, it more than doubles in comparison to the prereform level (8.7%).

The timing of primary school entry may have an effect on children's educational outcomes, although the direction of this effect is *a priori* unclear. On the one hand, being older at school entry entails being relatively more mature. If maturity at school entry matters in the long term, starting school earlier may have an overall negative effect on children's educational outcomes (Bedard and Dhuey 2006). However, starting school earlier may be an advantage if children learn more in school than at home or in preschool environments. For immigrant children, who have relatively high rates of social and economic disadvantages in their families, an earlier integration into the school system may indeed be beneficial. Moreover, parents may be encouraged or even forced to become more involved in their children's education if children are enrolled earlier in school. Because of data limitations, we are not able to provide a comprehensive analysis of children's educational outcomes during primary school. However, the data allow us to examine the effects of the reform on the probability of grade retention. Panel B shows a sizable postpolicy reduction in the probability of grade retention among immigrant children. The point estimate from our preferred specification indicates a reduction in grade retention by 5.8 percentage points, or 24.5% of the prereform level. This effect reduces the immigrant-native gap in grade retention (10.3 percentage points; see table 3) by more than one-half.

<sup>29</sup> One explanation for the statistically weaker effect on school readiness might be that pediatricians use a threshold model to assess school readiness, and small improvements in some developmental measures may thus be insufficient to push some children past the threshold.

**Table 6**  
**Reform Effects at the Primary School Level**

	(1)	(2)
A. Age at Primary School Start		
Age at school entry [baseline = 82,915]	−1.566*** (.345)	−1.574*** (.347)
<i>N</i>	1,618	1,618
Early school entry [baseline = .087]	.108*** (.021)	.108*** (.022)
<i>N</i>	1,626	1,626
B. Educational Progress		
Grade retention [baseline = .237]	−.056* (.029)	−.058* (.029)
<i>N</i>	1,607	1,607
Birth months	Yes	Yes
Child characteristics		Yes

NOTE.—Ordinary least squares estimates of eq. (1) using the school register (SR) data from 2009 and 2010. Mean of dependent variable for children born between July and December 1999 reported in brackets. Child characteristics include gender. Family characteristics are not available for the SR data. Standard errors are clustered at the birth-month/year level.

\* 10% significance level.

\*\*\* 1% significance level.

### C. Secondary School

Table 7 shows the reform effects at the secondary school level, specifically in fifth grade. For this analysis, we restrict the sample to children who have progressed to secondary school on schedule (81%). We discuss the results of specification 2, which controls for the full set of month-of-birth dummies and children's gender.

The results in panel A show that the likelihood of immigrant children receiving a recommendation for the academic track of secondary school was positively, but not significantly, affected by the reform.<sup>30</sup> Recall, however, that in the state and period under study, the ultimate decision of which track a child should follow in secondary school was at the discretion of parents, with children likely involved in the decision-making. As such, the reform effect on actual track choices may be stronger than the effect on teachers' track recommendations. Panel B shows this to indeed be the case. In particular, the reform increased the share of immigrant children attending the academic track of secondary school by 9.5 percentage points, or 38.9% from the prereform mean. This, in turn, implies a reduction in the academic-track enrollment gap between native and immigrant children by 48.0%. We next ask whether this effect is driven by immigrant parents being more likely to comply with teachers' recommendations for Gymnasium. Alternatively, are

<sup>30</sup> Our estimates indicate an increase in academic track recommendation by 4.3 percentage points, or 28.5% from the prereform mean.

**Table 7**  
**Reform Effects at the Secondary School Level**

	(1)	(2)
A. Recommendation		
Gymnasium recommendation [baseline = .065]	.043 (.027)	.043 (.027)
<i>N</i>	1,320	1,320
B. Track Choice		
Gymnasium [baseline = .174]	.095** (.035)	.095** (.035)
<i>N</i>	1,320	1,320
Gymnasium with recommendation [baseline = .066]	.068** (.032)	.068** (.032)
<i>N</i>	1,153	1,153
Gymnasium without recommendation [baseline = .123]	.059* (.030)	.060* (.030)
<i>N</i>	1,169	1,169
Birth months	Yes	Yes
Child characteristics	Yes	Yes

NOTE.—Ordinary least squares estimates of eq. (1) using the school register (SR) data from 2009 and 2010. The estimation samples include only children attending secondary school. Estimations regarding children's enrollment in gymnasium with and without recommendation drop the respective other group (i.e., estimations regarding children's gymnasium enrollment with recommendation drop children enrolled in gymnasium without recommendation and vice versa). Mean of dependent variable for children born between July and December 1999 reported in brackets. Child characteristics include gender. Family characteristics are not available for the SR data. Standard errors are clustered at the birth-month/year level.

\* 10% significance level.

\*\* 5% significance level.

immigrant parents more likely to override teachers' recommendations and send their children to Gymnasium despite the lack of a recommendation? We find both scenarios to be relevant. On the one hand, the proportion of immigrant children attending Gymnasium with an official recommendation increases by 6.8 percentage points, or 103.0%. On the other hand, the share of immigrant children attending Gymnasium without an official recommendation increases by 6.0 percentage points, or 48.0%.

In summary, we have found positive effects of birthright citizenship in the first three stages of the education system. From this, we conclude that the introduction of birthright citizenship had a lasting impact on immigrant children's educational integration—that is, the effects we observe early on in children's educational careers do not fade out over time.

#### D. Sensitivity Analysis

We subject our results to several robustness checks. We first restrict our samples to children born before the reform was ratified by narrowing the window around the cutoff date (from 12 to 8 months). We then implement a donut strategy by dropping the months just around the cutoff date (i.e.,

December and January). Next, we artificially boost the prereform sample by 10% to account for possible return migration and consider two scenarios for the newly generated observations: a lower-bound scenario, in which we assume the worst possible educational outcome, and an upper-bound scenario, in which we set the outcome equal to native prereform mean. Finally, we draw on one additional reform cohort and control flexibly for possible time trends by interacting being born between January and June with each birth cohort.

Table 8 presents the results. The following points are worth noting. That our results are robust to specifications 1 and 2 indicates that adjusted fertility behavior is unlikely to be a concern in our setting. The estimates for immigrant children's participation in preschool, language proficiency at the end of preschool, primary school starting age, and secondary school track choice remain unchanged irrespective of the sample restriction. The estimates related to teachers' recommendation regarding children's readiness for primary school and secondary school track choice lose magnitude after restricting the sample to an 8-month window around the cutoff date—a result suggesting that increased parental investment is the driving force behind the decision regarding immigrant children's early school start and access to the highest educational track. The results also remain robust when assuming extreme scenarios, a lower-bound and an upper-bound scenario, regarding attrition due to return migration (see cols. 3 and 4, respectively). Finally, estimates based on three cohorts and controlling flexibly for possible time trends (see col. 5) are also comparable in size and precision. This suggests that there are unlikely any further differences between children born in different semesters of a school year that could bias our results.

Recall that our estimates for the preschool level and those for the primary and secondary school level are based on different samples. Although there are no major differences between the SEE and SR sample in terms of parents' socioeconomic status (see tables 4, A2), the SR sample contains exclusively immigrant children who do not speak German at home. We now repeat our analysis of secondary school track choices with auxiliary data from the GMC. The GMC has two advantages over our administrative data. First, we can draw a sample of second-generation immigrant children based on information about their own as well as their parents' country of birth. Second, the GMC contains the information necessary to construct children's eligibility status. However, the GMC is very restrictive in other respects. First, as for educational outcomes, it only contains secondary school track choice. Second, due to a rather small sample size, we cannot distinguish between different federal states and thus between those with binding and nonbinding teacher recommendations.

Our main results from the GMC, reported in table A3, are as follows. In the baseline sample, which contains both eligible and ineligible immigrant children, we find that the reform led to an increase (albeit not significant)

**Table 8**  
**Robustness**

	Eight-Month Window (1)	Donut (2)	Lower Bound (3)	Upper Bound (4)	Trend (5)
<b>A. Preschool</b>					
Preschool enrollment	.033** (.013)	.042*** (.012)	.114*** (.011)	.029*** (.010)	.034*** (.013)
<i>N</i>	3,046	3,713	4,865	4,865	6,740
Language proficiency	.037*** (.010)	.041* (.024)	.035 (.024)	.025 (.026)	.035** (.016)
<i>N</i>	841	1,019	1,573	1,573	2,730
Socioemotional maturity	.021* (.014)	.007 (.011)	.116*** (.012)	.020* (.010)	.029** (.012)
<i>N</i>	2,306	2,813	3,766	3,766	5,169
School readiness	.015 (.014)	.026 (.028)	.092*** (.014)	.010 (.014)	.006 (.016)
<i>N</i>	3,046	3,713	4,865	4,865	6,740
<b>B. Primary School</b>					
Early school entry	.066*** (.022)	.132*** (.020)	.099*** (.019)	.092*** (.019)	.109*** (.024)
<i>N</i>	1,048	1,355	1,748	1,748	2,587
Grade retention	-.074* (.041)	-.031 (.031)	-.058* (.030)	-.037 (.027)	-.056* (.030)
<i>N</i>	1,035	1,338	1,729	1,729	2,555
<b>C. Secondary School</b>					
Recommendation	.025 (.034)	.079*** (.026)	.052** (.025)	.004 (.025)	.043 (.030)
<i>N</i>	843	1,090	1,442	1,442	2,010
Gymnasium	.074* (.037)	.122*** (.039)	.110*** (.014)	.094*** (.033)	.093*** (.039)
<i>N</i>	843	1,090	1,442	1,442	2,010
Gymnasium with recommendation	.033 (.030)	.105*** (.032)	.074** (.029)	.036 (.029)	.067* (.035)
<i>N</i>	740	959	1,275	1,275	1,772
Gymnasium without recommendation	.063 (.040)	.058* (.035)	.066** (.013)	.057** (.028)	.058* (.034)
<i>N</i>	748	952	1,291	1,291	1,777

NOTE.—Ordinary least squares estimates of eq. (1) using the school entry examination (SEE) 2005 and 2006 and the school register (SR) 2009 and 2010; the estimates shown in specification 3 draw additionally on the SEE 2007 and the SR 2011. Control variables include age and gender. Standard errors are clustered at the birth-month/year level.

\* 10% significance level.

\*\* 5% significance level.

\*\*\* 1% significance level.

in immigrant children's enrollment in Gymnasium by 5.9 percentage points, or 23.1% from the prereform mean. If we restrict this sample to eligible children, the effect increases to 6.7 percentage points, or 30.7% from the prereform mean. We consider this suggestive evidence that our results for the secondary school level are not specific to the subgroup of immigrant children contained in the SR sample.

Finally, in interpreting the results on Gymnasium attendance (see table 7), it is important to bear in mind that the SR sample only includes children who have progressed through primary school on schedule, that is, it does not include "latecoming" children who started primary school with a delay or had to repeat a grade. Because the reform had an impact on age at school entry and grade retention, it has affected the composition of this sample. To address potential concerns about endogenous sample composition, we have reestimated the reform effect on Gymnasium attendance using SR records from 2012 and 2013. By that time, the cohorts we study were scheduled to attend eighth grade, and all children, including latecomers, should have been tracked into secondary school. Thus, endogenous sample composition is not a concern here. Furthermore, the exercise tells us whether the reform effect on children's tracking outcomes at the start of secondary school persists at higher grades. The results, reported in table A4, show this to be the case: when using the SR records from 2012 and 2013, we still find a significant reform effect on Gymnasium attendance of 5 percentage points.

#### E. A Comparison to Estimates in the Literature

Opponents of birthright citizenship have described it as a "magnet for illegal immigration,"<sup>31</sup> cited the risk of national identity dilution (Huntington 2004; Wilcox 2004; Jahn 2014), and raised concerns about a shift in future voter composition (Razin, Sadka, and Suwankiri 2014). In contrast, proponents have argued that it is one of the most powerful mechanisms of social inclusion.

Our results suggest birthright citizenship to be a policy with substantial benefits. To see this, let us compare our findings with those of recent studies investigating alternative early-life interventions, especially universal preschool and targeted tutoring. Cornelissen et al. (2018) investigate the consequences of introducing universal preschool in Germany in the late 1990s and early 2000s on children's development at the onset of mandatory schooling. Using data from SEEs, they find that preschool attendance closes the immigrant-native gap in school readiness on average. In comparison, birthright citizenship closes the immigrant-native gap in school readiness by one-quarter and by more than one-half when scaling our estimates up to the average treatment effect (recall that the scaling factor amounts to 2.3 in the case

<sup>31</sup> See, e.g., a recent article at Zenit (<https://de.zenit.org/articles/rechtspopulismus-in-italien-verbreitung-einer-kultur-der-angst/>).

of the SEE data). In a similar vein, Currie and Thomas (1999) show that participation in Head Start closes one-fourth of the gap in test scores between Hispanic children and non-Hispanic white children and two-thirds of the gap in the probability of grade retention. In comparison, birthright citizenship closes the immigrant-native gap in grade retention by one-half and fully when considering the average treatment effect of birthright citizenship (recall that the scaling factor for the SR sample ranges between 1.8 and 2.5). Finally, Carlana, Ferrara, and Pinotti (2018) provide evidence for the integrative impact of a randomized tutoring and career counseling program provided to high-ability students in Italy. In the case of boys, it led to a closure of the immigrant-native academic track enrollment gap. The effect of birthright citizenship is comparable: on average, it closes the immigrant-native enrollment gap by roughly one-half and fully when scaling our results up to the average treatment effect.

Taken together, the effects of birthright citizenship are comparable to those of direct educational interventions, such as universal preschool and targeted tutoring, but is arguably associated with much lower costs. Indeed, the main direct costs of introducing *ius soli* in Germany were administrative, which were low given that citizenship was simply recorded on a child's birth certificate. By contrast, the educational interventions discussed above involve direct costs such as the hiring of new personnel, the construction or expansion of childcare facilities, or the training of tutors. For example, in Germany the costs of a preschool slot amount to approximately 850 euros per month and, thus, to more than 30,000 euros per child who attends preschool between ages 3 and 6. The costs of Head Start in the United States came to 7,600 USD per child per year, while the Italian tutoring scheme added up to 2,177 euros per student.

## VI. Concluding Comments

Successfully integrating children with migration backgrounds into the education system is high on the policy agenda in many countries. However, surprisingly little is known about the causal factors underlying immigrant children's educational integration. In this paper, we have taken some steps to fill this gap by evaluating whether a major citizenship reform in Germany—one that saw the introduction of birthright citizenship—helped to close the immigrant-native gap in a series of education outcomes over the early life cycle. Theoretically, a link might be expected because granting citizenship at birth can be viewed as a positive shock to the long-run rate of return on investments in children's human capital. However, in practice the effect is far from being clear: while an initial increase in parental investments may stimulate children's development and thus provoke continuous parental and child effort, a lack of an echo in official evaluations and success may discourage parents and children from maintaining their efforts and

thus have no result on children's educational outcomes in the long run. We address this question empirically and exploit the birth date cutoff determining whether a child became eligible for birthright citizenship. This allows us to overcome problems of endogeneity by using a difference-in-differences approach: our treatment group comprises children born shortly before and shortly after the reform's cutoff date, while children from the preceding birth cohort are used as the control group.

Our results show that the introduction of birthright citizenship had sizable positive effects on the educational integration of immigrant children in the first three stages of the German education system. In particular, the policy (i) increased immigrant children's participation in noncompulsory preschool education, (ii) had positive effects on key developmental outcomes (e.g., German language proficiency) measured at the end of the preschool period, (iii) caused immigrant children to progress faster through primary school, and (iv) enabled them better access to the academic track of secondary school, a precondition for higher education and university studies. Our interpretation of these results is that the introduction of birthright citizenship has incentivized immigrant parents to provide their children with more similar educational opportunities to children in native families.

Our study offers some lessons for policy makers and raises interesting questions for future research. Granting citizenship rights to immigrant children in places where they are poorly integrated into the education system might be an effective policy lever for reducing educational disparities. Indeed, our results suggest that birthright citizenship helps close the immigrant-native gaps in key measures of educational success. While our analysis provides a range of insights, much remains to be done to understand the longer-run effects of birthright citizenship. The cohorts born around the German citizenship reform will soon enter the labor market, form their own families, and participate in civic life. Understanding the long-run effects of birthright citizenship in these domains presents an important and rich agenda for future research.

## Appendix

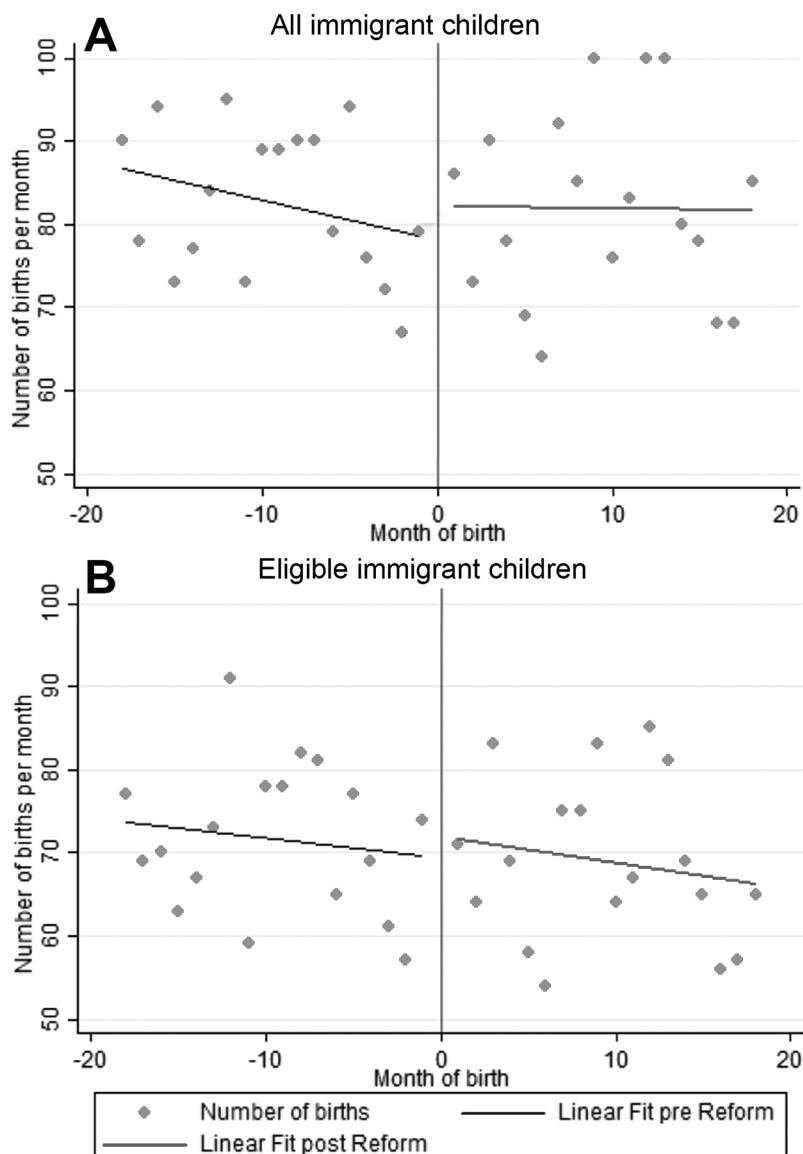


FIG. A1.—Number of births by month of birth. Source: German microcensus 2009–2012. A color version of this figure is available online.

**Table A1**  
**Sample Comparison based on Programme for International Student Assessment (PISA) Data**

	SEE Baseline	Sample Analogue	SR Sample Analogue
<b>A. Sample Restrictions</b>			
Born in Germany	1.00	.88	1.00
Main language is German	.54	.50	.00
<b>B. Family Background</b>			
Mom is born in Turkey	.25	.23	.27
Mom is born in the Soviet Union	.25	.27	.15
Mom is born in Poland	.09	.09	.05
Mom is born in the Balkans	.07	.07	.06
Mom is born in Italy/Greece	.03	.04	.06
Mom is born in Germany	.00	.00	.20
Dad is born in Turkey	.27	.25	.26
Dad is born in the Soviet Union	.24	.26	.15
Dad is born in Poland	.09	.09	.04
Dad is born in the Balkans	.06	.06	.07
Dad is born in Italy/Greece	.05	.06	.07
Dad is born in Germany	.00	.00	.20
Mom education: compulsory	.37	.37	.37
Mom education: more than compulsory	.24	.24	.24
Dad education: compulsory	.39	.38	.36
Dad education: more than compulsory	.26	.25	.24
<i>N</i>	372	422	254

NOTE.—Own calculations based on PISA 2015, which contains children belonging to the birth cohort 1999. We restrict the sample to children born between July and December 1999. The baseline sample includes all children of foreign-born parents who were born in Germany themselves. The school entry examination (SEE) sample analogue further includes children of foreign-born parents who were not born in Germany. The school register (SR) sample analogue consists of children born in Germany who speak a language other than German at home.

**Table A2**  
**Summary Statistics: German Microcensus**

	Prereform			Balancing	
	All Migrants II/99	Eligible Migrants II/99	Gap	All Migrants I/00–II/99	Eligible Migrants I/00–II/99
A. Dependent Variables					
Gymnasium	.255	.218	−.037	.022	.060
B. Background Variables					
Age in months	176.294	176.483	.189	−6.264***	− 6.549***
Female	.424	.411	−.001	.074	.034
Number of siblings	1.560	1.629	.029	−.113***	−.129***
Single parent	.185	.145	−.040	.035	−.015
Mom's education: low	.293	.323	.030	.046	.002
Mom's education:					
intermediate	.337	.298	−.039	−.064	−.036
Mom's education: high	.136	.129	−.007	.012	.017
Dad's education: low	.304	.371	.067	.011	−.021
Dad's education:					
intermediate	.190	.161	−.029	.044	.008
Dad's education: high	.120	.129	.009	−.005	−.005
Mom's origin: Turkey	.304	.419	.104***	−.060	−.098
Mom's origin: post-Soviet	.109	.121	.012	−.032	.025
Mom's origin: Balkan	.152	.113	−.039	−.056	.025
Mom's origin: EU 12	.087	.113	.026	.006	−.013
Mom's origin: other	.266	.129	−.137***	.064	.039
Dad's origin: Turkey	.283	.395	.112***	−.058	−.081
Dad's origin: post-Soviet	.076	.113	.019	.020	.011
Dad's origin: Balkan	.130	.113	−.017	−.049	.039
Dad's origin: EU 12	.033	.048	.015	.044	.047
Dad's origin: other	.245	.113	−.132***	−.009	−.048

NOTE.—Own calculations based on the German microcensus waves 2010 and 2011. “All Migrants” refers to children born in Germany and whose parents were both not born in Germany. “Eligible Migrants” refers to the restricted group of migrant children whose parents fulfilled the residency criterion at the child's birth. “Prereform” refers to children born between July and December 1999. The last two columns (“Balancing”) report the results of balancing tests regarding all/eligible migrant children born in the 6-month window before and after the cutoff date (January 1).

\*\*\* 1% significance level.

**Table A3**  
**Evidence from the German Microcensus**

	(1)	(2)	(3)
A. Baseline: Eligible and Ineligible Children			
Gymnasium [.255]	.065 (.050)	.059 (.052)	.059 (.042)
N	1,239	1,239	1,239
B. Eligible Children			
Gymnasium [.218]	.082** (.036)	.075** (.037)	.067* (.037)
N	821	821	821
Birth months	Yes	Yes	Yes
Child characteristics		Yes	Yes
Family characteristics			Yes

NOTE.—Ordinary least squares estimates of eq. (1) using German microcensus waves 2010 and 2011. Mean of dependent variable for children born between July and December 1999 reported in brackets. Child characteristics include gender and age quarter dummies. Family characteristics include number of siblings, a dummy for single-parent household, parents' educational degree, and parents' country of origin. Standard errors are clustered at the birth-month/year level.

\* 10% significance level.

\*\* 5% significance level.

**Table A4**  
**Evidence from School Register Data from 2012 and 2013**

	(1)	(2)
A. Recommendation		
Recommendation for Gymnasium [.070]	−.002 (.016)	−.002 (.016)
N	2,139	2,139
B. Track Choice		
Gymnasium [.109]	.050*** (.012)	.050*** (.012)
N	2,139	2,139
Gymnasium with recommendation [.057]	.011 (.014)	.011 (.015)
N	1,997	1,997
Gymnasium without recommendation [.059]	.046** (.013)	.047** (.012)
N	1,984	1,984
Birth months	Yes	Yes
Child characteristics		Yes

NOTE.—Ordinary least squares estimates of eq. (1) using the school register data from 2012 and 2013. Mean of dependent variable for children born between July and December 1999 reported in brackets. Child characteristics include gender. Family characteristics are not available for the school register data. Standard errors are clustered on a birth-month/year level.

\* 10% significance level.

\*\* 5% significance level.

\*\*\* 1% significance level.

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